DOCUMENT RESUME

ED 045 621 24 SP 004 615

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TITLE Research-Based Development: A Strategy for Educational Change in the 70's.
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SPONS AGENCY Office of Education (DHEW), Washington, D.C.
BUREAU NO BR-6-2931
PUB DATE 70
CONTRACT OEC-4-7-062931-3064
NOTE 18p.; Paper presented at the annual conference of the Social Science Education Consortium, Denver, Colorado, June 13, 1970

EDRS PRICE EDRS Price MF-$0.25 HC-$1.00
DESCRIPTORS *Educational Change, *Educational Development, *Educational Innovation, *Educational Research, Educational Resources, National Programs, Teacher Role

ABSTRACT

Of the three change strategies for educational improvement which are currently being supported with substantial sums of money, only the most recent, research-based development, can bring about real national improvement in educational practice. "Local innovation," although it will always be our best source of new ideas, fails as an effective national change strategy because of poor exportability, lack of rigorous evaluation, and incredible expense and inefficiency. The history of team teaching and the results of Title I of the Elementary and Secondary Education Act of 1965 illustrate the point. "Educational research" also fails as a change strategy, not because of scarcity of funds or a communications gap but because its findings as they come from the researcher are simply not usable by practitioners. "Research-based development" enlists both local innovation and research to produce new products and processes. Although it is costly, it is economically feasible when resulting products are widely used. It provides practitioners with products fully ready for use with rigorous research evidence that they do the job for which they were designed. If the new strategy is broadly implemented, the teacher's role can become more manageable. It would be more like that of a medical physician: working with each individual to diagnose his problem and select from available treatment the one considered most applicable--not to develop new treatments or discover new medicines. (JS)

ED045621

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RESEARCH-BASED DEVELOPMENT --

A STRATEGY FOR EDUCATIONAL CHANGE IN THE 70'S*

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INTRODUCTION

Today, perhaps to a greater degree than ever before, we urgently need an effective strategy for bringing about educational improvement. Three change strategies are currently being supported with substantial sums of money. I believe the time has come to take a critical look at these strategies, to review their past effectiveness, and to explore their future potential.

The first of these, which is as old as education itself, I will call the local innovation strategy. It is an experience-based strategy which assumes that if local school districts keep trying new approaches, eventually they will come across something that is better.

The second strategy is much newer, having been around for about 100 years. I will call it the educational research strategy. This, of course, is a research based strategy built on the premise that conducting research and, therefore, gaining new knowledge related to education will bring about improvement in educational practice.

The third strategy, relatively speaking is still in its infancy. With a few exceptions, it has been applied only in the past ten years or so. I will call it the strategy of research based development. This

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* This paper was presented at the annual conference of the Social Science Education Consortium, Denver, Colorado, June 13, 1970.

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strategy draws from both research and experience. It is based on the premise that educational improvement can be achieved by developing new educational products and processes using an R & D approach. In research and development, an educational product is continually field tested and revised until it is ready for operational use and has been proven to achieve the objectives that were set for it.

LOCAL INNOVATION

Let us first look at local innovation as a strategy for bringing about educational improvement. Local innovation will probably always be our best source of new ideas in education. But local innovation rarely provides more than the seed from which educational improvements may eventually flower. Few of the ideas of local innovation have ever developed into significant national improvements in education, because in the past we have had no effective way to nurture these ideas and develop them into usable products and processes.

If the idea of the local innovator is simple enough, such as using colored chalk instead of white chalk or laying carpet in the classroom instead of asphalt tile, it has some chance of gradually spreading and becoming a nationwide change. But what happens if the idea is not this simple? Team teaching provides a good example of a more complex change. Team teaching got its start in a few pilot districts in 1956 and 1957. These projects received substantial support from the Fund for the Advancement of Education, and by 1964 team teaching had spread to several hundred communities throughout the country (Shaplin & Olds, 1964). I conducted an extensive survey of team teaching in the Western States in 1966. The results

indicated that a majority of the 239 responding schools that had adopted team teaching had encountered serious problems in areas such as developing a satisfactory schedule, organizing teams, assembling needed materials and adapting the curriculum. Nearly half of the schools surveyed had no funds available for preplanning and many had abandoned team teaching after a year's trial (Borg & Brite, 1966). In spite of the many successful trials of team teaching, there was no comprehensive program that a principal could follow if he wished to implement team teaching in his school. There were certain basic requirements that he could learn about by visiting successful programs, attending workshops and reading books and articles. But translating these basic requirements into a workable team teaching program is a very difficult task. Thus, since each team teaching program represents a different application of the basic principles, we find that team teaching programs differ greatly. Unfortunately, however, we also find that many team teaching programs fail and are abandoned simply because many schools are not up to the task of building a program from the scant foundation available to them in the literature.

The history of team teaching is fairly representative of the deficiencies of local innovation as a strategy for bringing about significant improvement in educational practice. Perhaps its most serious defect is that local innovators almost never develop an idea to the point where it can be easily adopted by others. Some innovations, in fact, make such great demands and offer so little help to the prospective user that they are never used successfully by anyone except the originator himself. But even the most widely disseminated innovations such as team teaching fall far short of giving the potential user the help that is needed to make the innovation work effectively.

Poor exportability, however, is not the only reason that local innovation usually fails to bring about significant educational improvement. A second reason is that local innovations are almost never subjected to any sort of rigorous evaluation. Thus, for most innovations we do not really know whether the new approach is better than the old. Innovations are usually adopted not because they are better, but because they are different. Of course, after a year or two they are no longer different so the practitioner heeds the call of a new prophet and adopts a new idea - again something that is different but not necessarily better. This, of course, is fadism and often leads to cycles not unlike those found in women's fashions. Many so-called new ideas are discovered, tried, abandoned and later rediscovered. For example, ability grouping has undergone three such cycles in the past 75 years.

Perhaps the best reason for abandoning local innovation as national strategy for improving education is that it is incredibly inefficient and expensive. We need look no further than Title I of the Elementary and Secondary Education Act of 1965 to illustrate this point. The proponents of Title I assumed that if you gave local schools enough money they would build new programs and bring about improvement in education. To date Title I has provided us with a shocking six billion dollar test of this assumption. Although it is not easy to get quantitative data on Title I projects (since most of them have collected little or no valid research evidence), what information we do have indicates that these projects have brought about virtually no real improvement in the schools. Of course, no program that involves thousands of projects and spends billions of dollars can be completely barren, but it is sadly apparent that Title I had produced tons of chaff for every ounce of wheat.

Headstart, the panacea so fervently embraced a few years ago by that vast army of fuzzy thinkers we are cursed with in education, has similarly proven that a thousand zeros still add up to zero. Headstart, like the proverbial elephant, has labored mightily and brought forth a mouse.

In his message on educational reform of March 3rd (1970), the President has finally confessed what most hard-headed educational scholars and researchers have suspected for years. He said:

> "We must stop letting wishes color our judgments about the educational effectiveness of many special compensatory programs, when--despite some dramatic and encouraging exceptions--there is growing evidence that most of them are not yet measurably improving the success of poor children in school...
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> "Recent findings on the two largest such programs are particularly disturbing. We now spend more than $1 billion a year for educational programs run under Title I of the Elementary and Secondary Education Act. Most of these have stressed the teaching of reading, but before-and-after tests suggest that only 19% of the children in such programs improve their reading significantly; 13% appear to fall behind more than expected; and more than two-thirds of the children remain unaffected -- that is, they continue to fall behind. In our Headstart program, where so much hope is invested, we find that youngsters enrolled only for the summer achieve almost no gains, and the gains of those in the program for a full year are soon matched by their non-Headstart classmates from similarly poor backgrounds."

If we are to make the 70's a decade of real educational progress, we must put local innovation in proper perspective. Small-scale local innovation by a few creative teachers can provide the ideas which R & D workers can develop into significant educational products that can be distributed nationally. On the other hand, large-scale attempts to legislate educational improvement through local innovation, such as Title I, without doubt constitute the most wasteful and unproductive strategy that has ever been devised.

EDUCATIONAL RESEARCH

Let us now look at educational research as a strategy for bringing about educational improvement. When I was a graduate student in education nearly 25 years ago most scholars in education considered research to be the only approach that was likely to bring about real improvement in educational practice. True, there were not many improvements we could point to that were a direct outcome of research, but we had ways of rationalizing this apparent failure. One reason we gave was the almost complete absence of money to support educational research. Little research was being done and thus we could hardly expect more than a little improvement. It is certainly true that there was little money for educational research, but did this really explain why the research that was being done had so little impact on school practice?

Most of us believed that the real stumbling block of educational research was the communication gap between the researcher and the practitioner. It was obvious that most researchers communicated primarily with other researchers and used terminology which the school administrators of that time found obscure and meaningless. Both the researchers and the practitioner bemoaned this communication gap, and each blamed it on the other.

But do the scarcity of research funds and the communications gap really explain why research findings were failing to bring about educational change? Let's look at some evidence pertinent to this question. First consider the Cooperative Research Program established under the National Defense Education Act of 1955. Starting with a modest input of one million dollars in 1957, this act and subsequent legislation channeled a great deal of money into educational research during the decade of the 60's. During these same years

an increasing number of teachers and administrators were taking advanced degrees and becoming more sophisticated about educational research. Furthermore, major efforts to bridge the communication gap, such as the ERIC program, came into being.

But have these efforts resulted in a proportional increase in the application of research findings to the solution of school problems? I think not. In fact, I can find little evidence to suggest that there is any substantial increase in the direct application of research findings to the improvement of school practice. The reason for this sad state of affairs, I believe, is that educational research findings as they come from the researcher are simply not usable by the practitioner. No matter how effectively you communicate research findings to the practitioner, he will be unable to use most of them. Just as with local innovation, educational research is useful primarily as a source of raw material from which educational improvements can be built. Research findings per se are as far from a usable educational product as a silkworm is from a suit of cloths.

Have you ever picked an olive off the tree and tried to eat it? If you have, you know that olives are not fit to eat until they undergo a rather elaborate process. Similarly, most educational research cannot be consumed by the schools until it undergoes an elaborate process. This process is called research-based development or educational research and development. Although well established in science and industry, it is a new approach to improvement in education.

RESEARCH-BASED DEVELOPMENT

Because research-based development is relatively new in education, it is fitting to define this term and tell how it differs from educational

research. Research-based development refers to a process used in the development and validation of educational products. The essential element andthis process is the R & D cycle. This cycle consists of finding and studying research pertinent to a product to be developed, developing the product, testing it in the field to find its deficiencies and then revising to correct these deficiencies. In the more rigorous R & D activities, this cycle of field testing and revision is repeated until objective performance data indicate that the product brings about the educational outcomes for which it was designed.* The goal of research-based development, therefore, is to develop and test educational products to a point where their effectiveness has been scientifically established.

In contrast, educational research has as its goal, not the development of educational products, but the answering of specific questions (in the case of applied research) or the discovery of new knowledge (in the case of basic research). Of course, many applied research projects involve educational products. For example, if a project is concerned with the relative effectiveness of two methods for teaching reading, materials exemplifying the two methods must be developed. Typically, however, these materials are developed only to the point where they can be used to test the investigator's hypothesis. Thus, in educational research, the product (if any) is a means to an end while in educational R & D, the product is the end. For this reason, it is very rare for products to come out of educational research projects that are ready for operational use in the classroom.

There is then a crucial gap between educational research and educational practice. This gap is not primarily one of communication. It is the gap

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*Table 1 shows the main steps we carry out at the Far West Laboratory in the development of educational products.

between scientific knowledge and the ability to build usable products from that knowledge. Gaps between scientific knowledge and technology have existed in most sciences. But in the more mature sciences, methods of bridging this gap have long since been discovered. For example, the various kinds of engineers we find in industry are concerned primarily with the job of applying research findings in the physical sciences to the development of usable products. This is, of course, precisely the function that educational R & D workers perform for education and the behavioral sciences: they take the ideas and evidence generated by research and build tested products that areeready for use in education.

Development Costs

Our experience to date indicates that research-based educational development is a costly process. It is only economically feasible if the resulting product is used very widely. For example, one of our major programs at the Far West Laboratory is concerned with the development of minicourses. Minicourses are fully self-contained instructional packages designed to train inservice teachers in the use of specific teaching skills. Our instructional paradigm includes filmed model teachers who demonstrate the skills and microteaching so that the learner can practice the skills and receive videotape feedback. The R & D cycle we follow requires field testing and revising each minicourse at least three times and provides for the collection of pre- and post-course performance data to determine if teachers taking the course can perform the skills in their regular classrooms. A typical minicourse requires about 18 months to carry through our complete R & D cycle. During this time about seven thousand man-hours of effort are expended on the course and the direct development cost comes to about $100,000. If we take our

first Minicourse as an example, there are about 614,000 teachers in elementary schools in the United States who are at the appropriate grade levels for this course (Simon & Grant, 1968). If one out of every ten teachers takes this course, the development cost per teacher will be only $1.63. And in the case of Minicourse 1, there is a good chance that the federal money spent to develop this course will be returned to the U. S. Treasury. In fact, if the commercial publisher's market research is correct, the Treasury will receive royalties that exceed our development costs by about $160,000.* Thus, although the research-based development of educational products is expensive, this process is by far the most economical that we now have for bringing about significant nationwide improvements in educational practice.

In addition to cost-effectiveness, research-based development has several other advantages over both local innovation and research as a change strategy. First, it provides the educational practitioner with a product that is fully ready for operational use. Therefore, implementing a product that has undergone research-based development is much easier and much less likely to fail than implementing an innovation such as team teaching in which each user much re-invent most of what he needs to put his program into effect.

Second, a research-based educational development carries with it rigorous research evidence that it does the job for which it was designed. For example, to test the effectiveness of our Minicourses, we make videotape recordings of the teacher's actual classroom performance before and after the course. If these videotapes do not show significant improvements

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* Minicourse 1 is being marketed by Macmillan Educational Services, Inc., 8701 Wilshire Blvd., Beverly Hills, California.

in the teaching skills covered in the course, we continue to revise and field test the course until such gains are obtained. Only then do we release the course for operational use in inservice training programs. Thus, when the educator adopts a product that has been developed by the R & D process, he can expect an improvement in his educational program, not just a change. Since education is such a huge enterprise, even small educational improvements, when applied widely, can bring about cost effectiveness gains worth millions of dollars. If, through research-based development, we can put together enough small improvements we can bring about a quiet revolution in our schools.

Another important advantage of research-based development is that it provides an effective means of enlisting both local innovation and educational research in the task of building a better educational system. For example, if a promising local innovation is subjected to research-based development and proves to be effective, it can become a lasting national improvement instead of a passing local fad. Furthermore, the research knowledge that in the past has made little impact on educational practice can make an important contribution if it is used as the raw material out of which a better educational product is built.

Our few years of experience with research-based educational development indicates that we finally have a process that can bring about real improvements in educational practice. Since this approach is new to education, many of the products developed to date, such as some of the new mathematics and science curriculums, have been built using an inadequate R & D process in which essential steps have often been overlooked. For example, most of these curriculum developments initially failed to include sufficient training

materials to prepare teachers to use the curriculum effectively. This failure, of course, is a violation of the R & D principle that a product should include everything needed to use it effectively in an operational situation. The result of this oversight was that such curriculums were often badly misused by teachers. However, most of the early developers have now gone back and corrected this serious deficiency. On the other hand, most of the current development programs such as the SCIS Elementary Science Curriculum, the Minicourse, the Southwest Regional Laboratory's Reading Program and the IPI Curriculum, are using the R & D process more skillfully to build products that are of proven effectiveness and that contain everything needed for implementation.

The Teacher's Role

If I am right, and the decade of the 70's sees the development of a wide variety of research-based educational products, what will be the role of the teacher?

Before I describe this future role, let us look at the teacher's role in today's schools. Most school administrators still regard local innovation as the only means of achieving improvement, and they have given the teacher an impossible and extremely frustrating task. She is expected to assume a major responsibility for district curriculum building, to plan innovative and exciting lessons, to combine fragmentary and incomplete sources such as textbooks into effective instructional programs, and to diagnose each child and adjust the curriculum to meet his needs. And when are all of these difficult tasks to be accomplished? The average teacher is busy in the classroom about six hours each day and must devote another hour or two to such things as paper grading, clerical work, and playground duty.

This leaves a little time in the evening for minimal preparation of tomorrow's lessons, and no time at all for lofty tasks such as developing a new curriculum.

This vision of the teacher as a superman is a romantic one which appeals to many adolescents who contemplate a teaching career. But like many false visions, it is also cruel and probably accounts for our loss of many bright young people who abandon teaching each year with deep feelings of failure. One-third of students trained as teachers never enter the field and about another third leave the field forever within five years of graduation.

Thus, I suggest to you that the demands made upon teachers in most of today's schools are unrealistic, and they doom the more capable and idealistic teachers to disillusionment, frustration and failure. This role must be changed and no strategy for educational improvement can succeed unless it provides teachers with an achievable and gratifying role in the educational process.

An Analogy

In seeking a role for the teacher in the product-laden schools of the 70's, I would suggest we look to the medical profession for a model. Just what sort of role does the practicing physician play? He works with each patient as an individual, diagnoses his problem and selects from available treatments the one that he considers most appropriate. He is not expected to develop new treatments or discover new medicines. The treatments and medicines which the physician prescribes are usually developed and tested by medical researchers and scientists and are marketed throughout the world.

These products are thoroughly tested and except for occasional slip-ups, have been proven effective for the purpose for which they are sold. The doctor's basic task is to diagnose, prescribe and apply treatment, and it is in performing these tasks that he is regarded as a professional. If his diagnosis invariably led to the same treatment, he would be a mechanic. I am not proposing that teachers become educational mechanics, although the mechanic's role would be more productive than the role most teachers now fill.

I would suggest that the function of the educational practitioner should be similar to that of the medical practitioner. The main function of the teacher should be to diagnose the needs of each student, select from among proven educational products those that best meet those needs, and conduct necessary treatment. Diagnosis and prescription are, of course, difficult and will require training. But once the diagnosis and prescription are completed, the student is likely to be involved with the treatment for some time, thus giving the teacher time to diagnose and prescribe for other pupils. I believe this role is one that teachers can fill effectively and one which will give them a feeling of accomplishment and gratification.

Of course, teachers cannot assume this role today simply because the necessary inventory of proven educational products is not available. Therefore, the most important task in education today is to build a broad assortment of proven educational products and as these products become available, prepare teachers to use them.

This is a huge task. However, in educational research and development we have a process for accomplishing this task. All that we need now are time and money. The obvious place to get the money is from unproductive programs such as Title I. A large percentage of such funds should be diverted to

the support of rigorous, performance-based educational development programs. The remainder should be distributed to local schools with the stipulation that these funds be used only to purchase educational products of proven effectiveness. Within ten years our development work could reach a point where local schools would have available at least two or three products for each major educational objective. At this time available funds for development could be reduced and funds for local schools could be increased. To support and update such a system with new and improved educational products would require a continuous investment of perhaps ten percent of the national education budget. But the benefits would be tremendous. Virtually all children would receive an appropriate and effective education and we could expect the problems that have been created by today's ineffective and irrelevant educational programs to gradually disappear.

I am sure many of you feel that I am dreaming an impossible dream. But let me mention a few facts that suggest that the dream may be possible. First, consider the statements of political leaders that emphasize the need for developing and testing new educational approaches before spending money to implement them. Next consider the "accountability contracts" being entered into by a few publishing companies and private contractors which tie the cost of reading and mathematics programs to the results achieved. Finally, the new Title I guidelines have been directed more strongly than ever to evaluation of performance.

Let me close with another quotation from the President's March 3rd message on educational reform.

> "Mankind has witnessed a few great ages when understanding of a social or scientific process has expanded and changed so

> quickly as to revolutionize the process itself. The time has come for such an era in education."

It is my belief that the era he refers to has already dawned and the process that will bring about revolution in education is research-based development.

TABLE 1

THE MAJOR STEPS IN THE DEVELOPMENT CYCLE*

1.	Research and Data Gathering	Includes review of literature, classroom observations, and preparation of report on the state of the art.
2.	Planning	Includes definition of skills, statement of objectives, determination of course sequence, and small-scale feasibility testing.
3.	Developing Preliminary Form of Product	Includes preparation of instructional and model lessons, handbooks, and evaluation devices.
4.	Preliminary Field Test	Conducted by Laboratory personnel in one, two, or three schools, using between six and twelve teachers. Includes collection and analysis of interview, observational, and questionnaire data.
5.	Main Product Revision	Revision of product as suggested by preliminary field test results.
6.	Main Field Test	Conducted by Laboratory personnel in between five and fifteen schools using between thirty and one hundred teachers. Includes collection of quantitative data on teachers' pre- and post-course performances, usually in the form of classroom videotapes. Results are compared with course objectives.
7.	Operational Product Revision	Revision of product as suggested by the main field test results.
8.	Operational Field Test	Conducted by regular school personnel in between ten and thirty schools, using between forty and two hundred teachers. Includes collection and analysis of interview, observation, and questionnaire data.
9.	Final Product Revision	Review of product as suggested by operational field test results.
10.	Dissemination and Distribution	Reports at professional meetings, in journals, etc. Includes work with publisher who assumes commercial distribution, and monitoring of distribution to provide quality control.

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* Borg, W. R., Kelley, M. K., Langer, P., and Gall, M. The Minicourse - A Microteaching Approach to Teacher Education. Macmillan Educational Services, 1970.